EDITORIAL

'REALLY CRITICAL' GEOMORPHOLOGY

In a recent editorial and short communication in this journal, Keith Richards (1990, 1994) addressed some issues of scientific philosophy and suggested that future editorials should do the same. In particular he argued against the 'instrumentalist' view of research often presented to postgraduate students, where an emphasis on the critical rationalism championed by Haines-Young and Petch (1986) effectively reduces philosophy to method. Richards argues that a better philosophical underpinning for geomorphology is provided by a 'realist' philosophy of science, where 'the objective of science' is to develop explanations based on the identification of networks of underlying causal mechanisms' (Richards 1990 p. 195). I hesitate to enter this debate because I must admit that I, probably in common with most readers, have not read the original works of either Popper or Bhaskar, or the authors of the many other 'isms' to which Richards and others refer. However, I was heartened by Richards' (1994 p. 279) plea that we should avoid an arid discussion that turns on semantics and 'concentrate on how research is done, given a rather broadly defined philosophical position'. In that spirit I wish to make the simple point that if we want our science to progress then we need to be critical.

Richards attacks the current emphasis, particularly in postgraduate training, on hypothesis testing as a 'recipe for research' and stresses that the best research is often characterized by lateral thinking. I think two points are important here. First, hypothesis testing is not synonymous with critical rationalism. Second, those who do the very best research will probably do it with, without or despite any training, but the rest of us need all the help we can get. To me, science is about improving our understanding of the way the world really works. Theories and models are simply a way of expressing clearly and succinctly our current level of understanding. We do not improve our theories or models by admiring them, or by proclaiming how well they seem to fit our observations. The only way to improve them, and therefore to make progress, is actively to seek conflict between our models and the real world. Hypothesis testing is just one way to seek this conflict. However, the aim should not be the wholly negative 'refutation' of the theory that is proposed by my admittedly strawman view of critical rationalism, but the constant revision and improvement of theory. I do not mean that something should be added to the theory to protect it from the unexpected results of the hypothesis testing experiment, I mean that we should accept that the theory conflicts with reality and try to create something positive out of that conflict.

The best research is certainly characterized by imagination and lateral thinking, but the worst and even the mediocre is characterized by woolly thinking and an absence of theory. An emphasis on clearly defining models, theories and hypotheses, and on using theory to predict the likely outcome of experiments is a useful discipline. Teaching postgraduates (and others) how to define clear and testable hypotheses is a useful element of training and we should beware of throwing this baby out with the critical rationalist bathwater. Training should not of course end here, and Richards' fears about the theoretical underpinning of some, particularly computer-modelling based, Ph.D. projects are well placed. It is important to realise that hypothesis testing is not the only way to seek conflict between theory and reality. A theory can be brought sharply into conflict with reality by, for example, frank and open discussion with colleagues, a critical question raised at the end of a conference presentation, or by the perceptive comments of a good referee. I make this point in an editorial as the new BGRG editor because the BGRG and other groups have a very important role to play in shaping the way that science is done within our community. If we encourage critical debate then we encourage progress, but if we stifle criticism then we retard progress.

I have been disappointed in recent years at the lack of criticism that has been evident at, for example, the annual meetings of the BGRG. It is now unusual, though refreshing, to hear a really perceptive yet critical question raised after a talk. Perhaps this reflects simply good manners and an unwillingness to offend, but if that is true then it reveals an important weakness in our approach to science. When we present a paper at one of our conferences we should not regard it as the final word on the subject, to be admired and applauded but not questioned. On the contrary, the things

CCC 0197-9337/97/010001–02

that we deal with are so complicated that we can only hope to produce models that come closer to reality than someone elses. Our aim should be to see a particular branch of the science progress and the best way to achieve this is for someone to raise the possibility that there are parts of the theory that do not fit their perception of reality. Criticism should be seen as positive and helpful, and part of the role of BGRG should be to foster a community within which criticism is encouraged and desired, and in which it is not a terrible crime to get something wrong. History paints with a very broad brush, and most of us will eventually be shown to have been wrong to some extent.

In publication too there are ways in which we can advance our science through being more critical. Authors of articles in ESP&L, for example, might like to consider whether the structure of their paper adequately reflects the way that the research was really done. Most papers are still written in the 'inductive' style involving methods, results and conclusions, but there is no reason why we should not be more critical of this approach. Authors might also consider being open and direct about those areas of their work which conflict with the ideas of others. This greatly assists non-experts to identify the key areas of debate and of progress. Authors might also consider being more self-critical. If there are observations that do not fit your theory, and you are not sure why, then say so openly. Making these conflicts between theory and observation readily identifiable will help to accelerate progress. Finally, there is the role of the referees. The only way to maintain the quality of papers in ESP&L, and thus mark progress in the science of geomorphology, is through careful peer review. We need to weed-out those papers which are of insufficient quality, guide authors to present their material clearly and yet concisely, and at the same time beware of rejecting work which may be unusual, even potentially unpopular, yet may represent real progress. Reviewing a paper critically is a difficult and time-consuming task and I hope that our many referees, including members of the BGRG, will continue this vital role.

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